word about that divergence. I spent much time in counting the elements in the organ of Torpedo, and confirmed my results by counting also the ganglionic cells belonging to the plates. My total comes very near to that of Valentin, but amounts only to 179,625 in each organ.

Prof. Ewart cannot expect me to give up my number in favour of his, published much later, before he proves that I made a wrong estimate. He speaks also of the large Torpedo of America, and calls it *Torpedo gigantea.* It seems he is not aware of the fact that the name of *Torpedo gigantea* is given to the petrified species from the Monte Bolca, whilst the American species got the name of *T. occidentalis* (Storer). He ignores, or neglects, at the same time, the fact that a near related species, which has about the same number of electric elements, generally named *T. nobiliana* (Bonap.), occurs also in the British seas.

If it is difficult to explain such want of harmony with other authors; it amounts to an impossibility for a foreigner to give a clear account of the following papers concerning the cranial nerves of Elasmobranch fishes. Not that I mean to blame my learned colleague for this; on the contrary, I admire the papers very much, and recommend them with all my heart to everyone who wants instruction about the finer details of these nerves; but with regard to the nomenclature employed, I am afraid very few continental authors will agree with the homologies stated by Ewart. When he in a certain place says against Sappey, "that nerve is all but universally acknowledged to be a part of the facial," Sappey will most decidedly state "that nerve is all but universally acknowledged to be a part of the trigeminal."

Considering it of little use to discuss the confusion of names in the papers quoted by Ewart, I wish it to be borne in mind that the leading principle of the author to prove his homologies is the equality of distribution of the nerves in the peripheric organs. If that holds good, as I am convinced it does, how can he at the same time give the name of a true motor nerve (*N. facialis*) to a cephalic nerve of a true sensitive character? Perhaps he will answer, "All, or at least most, of the other authors do, why shouldn't I do the same?" Putting aside the protest many continental authors (myself included) make against such nomenclature, at any rate his principle of innervation is given up, and I am firmly convinced that the comedy of errors in the nomination of cranial nerves in comparative anatomy will not cease until quite new names or, perhaps still better, only numbers are applied to them according to the place of origin in the substance of the central nervous system. Motor and sensitive nerves must be kept separated by all means, segmental and not segmental nerves may be designated at the same time in any proper way.

It is in this respect that the want of an international understanding is most severely felt, and we must hope that the future may provide an advancement of science also in the matter; for before a firm and clear base for these homologies of nerves is given, we might just as well talk Chinese together.

Professor Ewart's investigations about the cranial nerves had for their chief purpose a clearer insight into the innervation of certain organs of sense, treated in another paper annexed to the same volume. I am very glad to state that the impossibility of accepting his homologies does not interfere with his results as regards the innervation of these sense organs.

The anatomical skill of the author is best shown in the treatment of the structure of these organs. So far as my own experience in these matters goes, I am led to ask, Are his statements and figures of the sensory canals and the nerves belonging to them very correct and complete? He overreaches the previous writers treating the same objects by the admirable finish of his papers which, as far as I see, ought to be followed by another concerning the ampullary canals.

In this chapter I have also to object to his way of treating the literature and of stating homologies in spite of his own principles.

The sense organs I discovered on the skin of Raja, and called "Spaltpapillen," named "pit-organs" by Ewart, were not found by any other author before. It is not true that Merkel saw them on the back of Mustelus and at the mouth of Spatina; he described only "freie Nervenhûgel" (*free* nervous collines) in these places. The name itself proves that the sense organs described by Merkel belong to another group altogether, and so does their position.

How Ewart, who places such importance in the distribution of the nerves, can find that the "Spaltpapillen" probably correspond to organs of Squatina placed at the mouth of a very different make and different innervation, he may know himself, but the reader finds it impossible to follow such argument.

The figure he gives of the pit-organ (sensory canals, pl. 3, Fig. 10) does not show such an organ fully developed. Otherwise the split would be narrow and straight. the cells by which it is lined flattened and columnar, not rounded, the papilla itself much higher raised above the surface of the epithelium, and pigment cells frequently found between the epithelial cells. (Comp. my paper: "Uber Bau und Bedeutung der Canalsysteme unter der Haut der Selachier." *Sitz. Ber. d. Berlin Akad. d. Wischenss*, 1888, s. 291.)

The papilla is, in the adult, a good deal raised above the level of the skin, so that even the sense organ at the bottom of the split in the papilla has still a somewhat elevated position. This position prevents me from admiring the name of "pit-organs," as a pit ought to mark a depression below the main level. Continental writers will also shake their heads in reading that a differentiated group of epithelial cells forming a sense organ, and resting between them, is called a "follicle," which by all means wants a kind of stronger envelope enclosing the cells. But I quite agree that in England Latin words might be admitted, which would not do on the continent.

Before Prof. Ewart proceeds to describe the ampullary canals, I recommend him once more to study the paper of Savi concerning these canals. He places the French author amongst those who take also the ampullary canals for sense organs, which is a great mistake, as Savi affirms in most decided terms the excretoric function of the ampullary canals. (Mateucci et Savi, "Études Anatomiques sur le Système Nerveux et sur l'Organe Electrique de la Torpille," p. 331.)

But such objections very slightly detract from the great merit of the author. They only prove the strong interest which the perusal of Prof. Ewart's papers has aroused in me, as it will do in all other readers. I cannot conclude my remarks without acknowledging once more, with all my heart, the magnificent results obtained by him, and I trust that he may succeed further in the same direction.

GUSTAV FRITSCH.

NAVIGATION BY SEMI-AZIMUTHS.[1]

THE year 1893 should be an interesting one to nautical men. A new Daniel has come to judgment in the person of Mr. Ernest Wentworth Buller, M.R.A.I., M.R.U.S.I., M.I.E.E., the inventor of the semi-azimuth system of navigation, who is equally earnest in denouncing the shortcomings of the existing systems of navigation, and in advocating the merits of his own. For the precise object of the new method let our author speak for himself. In page iv. of the preface we read: "It is here claimed for the semi-azimuth method that it renders double altitudes unnecessary; that a better result than they have been supposed to yield can be obtained from *either observation* singly, and this also with a great saving of time and trouble."

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[1] "Semi-Azimuths. New Method of Navigation, being a combination of Spherical Trigonometry and Mercator's Sailing." (London: Norie and Wilson, 1893.)

Again on page 27: "The range of the General Method extends, either from the meridian, or from the limit at which the direct method becomes uncertain, viz., three points from the meridian, up to an azimuth of seven points, or more, that is to say to within less than one point from the prime vertical, which may be considered the practical limit of the semi-azimuth method. . . . It may safely be affirmed that nothing like this extent of range in the computation of latitude has ever been attained by the current systems of navigation."

The feats which our new teacher claims to have accomplished are two in number.

(1) The discovery of a new formula for reduction to the meridian.

(2) By a somewhat tedious system of approximations to obtain by the semi-azimuth method from either of two observations for double altitude the same or a better result than would under the usual double altitude method be derived from the two combined, subject only to the limitation that the body must not be within a point of azimuth from the prime vertical.

Let us consider (1) and (2) in detail.

First with regard to (1), the formula obtained is this:

$$\text{Reduction} = h \cos l \times \text{arc} \quad \text{azimuth}.$$

The formula is a simple one, and now that hour angle is so easily converted into azimuth by the Burdwood and Davis' tables, there seems no objection to its adoption by those who prefer to work out their correction rather than to take it direct from the tables.

Mr. Buller deduces the formula from a somewhat elaborate construction on the Mercator's chart. It is, however, merely an old friend masquerading in a new dress, and he might, if he pleased, have easily obtained it from the expression in ordinary use.

Thus in the formula

$$\sin\frac{\theta}{2} = \sin p \sin c \operatorname{cosec} z \sin^2\frac{h}{2},$$

where θ is the correction, p the polar distance, c the colatitude, z the zenith distance, and h the hour angle, if we assume that

$$\frac{\sin\frac{h}{2}}{\sin\frac{A}{2}} = \frac{\sin h}{\sin A} = \frac{\sin z}{\sin p},$$

where A is the azimuth, we obtain

$$\sin\frac{\theta}{2} = \sin c \sin\frac{h}{2} \sin\frac{A}{2},$$

or

$$\theta = \tfrac{1}{2} \sin c \,.\, h \,.\, A \,.\, \sin 1',$$

the Buller formula in a logarithmic shape.

The use of this formula is first exemplified by its application to examples from Jeans' "Navigation," and so long as the azimuth is small it answers well enough. But our author is tempted further afield, and so soon as he gets well away from the meridian his formula begins to give trouble, and he is only able to obtain an accurate result by a process of approximation so cumbrous as to be quite useless for practical purposes. As an instance of this, witness the example of which the results, but not the full work, are shown on p. 14. Were the work to be shown in full, it would occupy more nearly two pages than one page of the text.

Now let us suppose it had not been given to Mr. Buller to discover the methods lately presented to the nautical world, and that he had been content to follow mere ordinary processes.

Imagine, for instance, that he had selected the well-known formula

$$\text{vers Mer Zen Dist} = \text{vers}\, z - \sin p \sin c \,\text{vers}\, h$$

in order to work out example § 16, p. 12.

Then, upon his own assumption that the latitude was 51°, or 12′ in error, he would obtain as a first result lat. 50° 49′ 42″. Repeating the process with this new latitude, a second approximation would be 50° 48′ 16″, while a third repetition would result in 50° 48′ 3″, the true value and this with less than one-third of the trouble.

The semi-azimuth had better, therefore, be confined to observations within a point or so of the meridian.

We pass on to the second and more important part of the task which Mr. Buller has set himself, namely, to show that the latitude within certain wide limits of azimuth may be obtained with accuracy from a single altitude without waiting for a second.

And this may be at once conceded, that by making the necessary adjustments for change of azimuth, and by successive approximations, an altitude may be reduced to the meridian, even when the azimuth is considerable.

But the same result may be obtained from the versine formula given above with far less trouble but greater accuracy.

The question, then, narrows itself to this: Why should we not in all cases of observation within seven points of the meridian, reduce the altitude to the meridian at once, by one method or another, and so obtain the latitude without waiting for hours to take a second altitude, and then making lengthy calculations?

Why have Robertson, and Raper, and Inman, and the other giants failed to hit the bull's-eye, while it is left to Mr. Buller to put his finger on the "blind spot"?

The answer is easily stated.

The "blind spot" is to be found not in the accepted custom of mariners, but in the author himself. Mr. Buller in arriving at his conclusions leaves out of consideration the real vital point which attaches to every observation taken ashore or at sea, but especially to the latter class, namely, what is well defined by Raper as the "Degree of dependence" to be placed on the observation.

In every observation off the meridian we have to deal in one form or another with a spherical triangle, in which three elements being given we have to find a fourth.

Now the three given elements are in general known only approximately, and it behoves us to find under what conditions an observation should be taken that the smallest possible error may be produced in the final result. In the problem under consideration, treated by the Buller process, the data are the polar distance, the approximate colatitude, and the zenith distance.

Of these the polar distance may fairly be regarded as accurately known, since the difference can only be at most but a few seconds.

The latitude is required to be known only approximately, the object of the observation being to find the amount it is in error, and it has already been admitted that the new process will suffice to obtain the correct latitude, *always supposing that the observed altitude is correct.*

What reason is there then to consider that the altitude is correct, and what will be the effect if it is incorrect?

When we take into account the haziness and uncertainty of the horizon, the difficulty of accurate observation on board a rolling ship, the varying effects of refraction, the imperfections of the sextant, and the personal error of the observer, it is probable that an average error of 2′ is a very moderate estimate.

Taking 2′, therefore, as an average value, let us see what will be its effect upon the typical examples in the text, worked first by the new process, and secondly by the old method of double altitude.

On page 30 an example (§ 32) is taken from Lecky, which will answer the purpose.

The data are as follows. Lat DR 32° 15′ N.

March 7, 1880.	Times by chron. h. m. s.	Altitude. ° ′ ″	Bearing. °
11h. a.m. ...	1 11 3·8 ...	50 0 0 ...	S. 25 E.
3h. p.m. ...	5 11 1·2 ...	33 17 45 ...	S. 57 W.

Sun's declination, 4° 59′ 15″ S. for first observation; 4° 55′ 30″ S. for second observation.

Let us suppose that an error of 2′ occurs in the second altitude, the one treated for reduction to the meridian.

The error in latitude (dl) produced by an error in altitude (dz) is given approximately by the formula

$$dl = \sec A_1 \cdot dz,$$

where A_1 is the azimuth of the body.

$$\text{Thus } dl = \sec 57^\circ \times 2', \text{ or } 3'\ 40'' \text{ nearly.}$$

But if treated, so as to take in both observations, by the double altitude process,

$$dl = \frac{\sin A_2}{\sin (A_1 + A_2)} dz,$$

where $A_1 = 57^\circ$, $A_2 = 25^\circ$, the azimuths being reckoned from south in each case. Thus

$$dl = \frac{\sin 25^\circ}{\sin 82^\circ} 2' \text{ or } 51''.$$

So that in one case a reasonable error in altitude gives an error in latitude of nearly 4′, in the other of less than 1′.

One other instance will perhaps suffice. On page 36 an example from Riddle is worked out, wherein an error of 2′ in altitude would produce an error of 18′ upwards in latitude.

Such a result at once condemns the observation. Indeed, for the purpose of accurate determination of latitude, the double altitude stands out among the various methods a very king. In other cases, as in the meridian altitude, we are satisfied if the latitude is no more in error than the original observation. In a double altitude, taken under advantageous conditions, only a fraction of the error in altitude appears in the final result.

It is somewhat remarkable that Mr. Buller's evident appreciation of Sumner methods has not made him more familiar with the main principles which apply equally to all classes of observations.

Every observation furnishes the observer with a circle upon the globe, a straight line upon the Mercator's chart, on which to place his position. The circle has the sun's projection on the earth for its centre. The line has the sun's line of bearing perpendicular to it.

If this line of position is inclined at a very acute angle to the meridian, that is, if the body observed is near the prime vertical, it is evident that a very small increase in the perpendicular drawn from the sun to the line of position, that is, a very small increase of zenith distance, will produce relatively a very large difference of latitude. And this condition, coupled with the impossibility of obtaining accurate altitudes at sea, is sufficient to account for the restriction of ex-meridian observations to a point or two in azimuth from the meridian.

The British seaman, therefore, had better pause before he throws overboard his Norie or his Raper, and takes to his heart the new Buller methods.

The greatest self-confidence, the most implicit belief in the reality of the mission to which he has been called, will not enable Mr. Buller to find the latitude accurately by a single altitude near the prime vertical, for the very simple reason that the error (even when supposed small) which must be expected in the altitude produces a large error in the latitude, and thus vitiates the result.

If he would make the ex-meridian method available as he proposes to do, at almost any time of day, the author must supplement his treatise by the invention of some appliance for measuring altitudes very much superior to any now in use.

Pending its production the very pertinent question asked in page 36, "We have known long enough how to get a fairly correct A. T. S. from an observation near the Prime Vertical and the latitude D. R., but who has yet shown how to obtain the *True Latitude*"? must remain unanswered.

There is indeed freshness in the "New Method of Navigation," Part I., but no light. That perhaps will be supplied by Part II.

G.

VOICES FROM ABROAD.

THE following literal translation of parts of an article recently published in the *Chemiker Zeitung* (Nos. 85 and 86, 1893) is an appropriate addendum to a recent article of mine in this journal. It must be sorrowfully admitted that in essential particulars the picture is a true one.

HENRY E. ARMSTRONG.

"Notwithstanding the enormous industrial development of England, the appreciation of science by technical workers is inconceivably slight, the main cause being deficient comprehension. The Englishman is conservative in all his customs, in his way of living, and not less in his methods of manufacturing, so that there are still very many manufacturers who would be as little prepared to place the control of their works in the hands of a scientific chemist as to convert them into philanthropic institutions. At present great efforts are certainly being made to alter this condition of affairs by the aid of technical schools modelled on German lines, but opinions as to the value of these schools are as yet much divided; and, indeed, for various reasons their ultimate success is doubted. In the first place, it is to be borne in mind that these institutions are not under State control, but are governed and controlled by local boards. Moreover, the preliminary training which their students have received is not to be compared with that of students in the German institutions, as an education such as is given in the German Realschulen and Gymnasiums—of the character given in England at most by the grammar schools—is only procurable by those who are well off, owing to the enormously high school fees (about £20 or 400 marks a year). The possibility of consolidating and widening the technical training by a short subsequent course of scientific study at the University is absolutely out of the question in most cases, owing to its extreme costliness. It is therefore probable that these schools will but produce a number of half-educated persons who will take up positions as chemists and will thereby but bring the chemist proper into discredit.

"It is clear that under these circumstances there is but very little prospect that a chemist coming to England will find a suitable position. I cannot sufficiently strongly caution 'young chemists' against coming to England on the chance of picking up something good, even when provided with good introductions. So few analysts are in demand here that the chance of securing such a post is most uncertain. Works and laboratories in which scientific work is systematically carried on scarcely exist, not one even of the English aniline colour works having a scientific laboratory worthy of the name. The 'young chemist' has therefore very little chance of securing an appointment, as he does not possess the necessary qualification for a works post, that is to say experience,